Chapter 6
Folder C134. Schumacher and Niels Bohr. January–March 1968

Jan 9, 1968

Dear Jeff

I received your two manuscripts.[1] They are, I think, much better than the original one. On the whole, I think that they could be published as they stand, with minor changes.

Firstly, the title is a bit misleading. You don't actually reconcile Bohr with Hidden Variables. A better title would be:

The Feature of Wholeness in Quantum Theory in Terms of Hidden Variables and in Terms of the Copenhagen Interpretation.

On P.6. you could perhaps consider the notion that Hilbert space with its probabilities is treated as a kind of "metalanguage". (This is due to Schumacher.) The "metalanguage" applies to the "language" of classical physics.

The feature of wholeness introduced by Bohr is to say that (because of the failure of classical mechanics) the description of experimental conditions and of experimental results is inseparably bound up in an unanalysable way. In classical physics, we could discuss, for example, the motions of planets entirely apart from a discussion of the instruments for observing these motions. In quantum theory, the conditions needed to observe momentum are inseparable from the "existence" of a state of well-defined momentum. These conditions are incompatible with those needed to define position (whereas in classical theory, no such incompatibility arises). As a result, apparatus and observed system are conceptually inseparable. Nevertheless, linguistically, we still separate them. So the separation is epistemological, rather than ontological. We do not do as Heisenberg, and portray electron and microscope as two separate objects. (I think you should emphasize in your discussion of Heisenberg that he is actually using the classical ontology in a confused way – confused

[1] Published as Bub (1968b, 1969)—CT.

C. Talbot (ed.), *David Bohm's Critique of Modern Physics*,
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because he also wants explicitly to deny it, in the very same argument in which he tacitly asserts it.) Rather, there is only one quantum object – the electron. However, it is described through the quantum metalanguage, whose "objects" are the terms of the classical "language" (whose objects are in turn the observable structure-function of the whole experiment).

One could perhaps think of the relationship between a book and a book review here. The "objects" referred to in the book are the people, events, places, described there. The book review refers to the structure of language and ideas appearing in the book. So there are two kinds of "objects": (a) People, trees, buildings, etc., along with events that describe what happens to them. (b) Novels, histories, expository books, children's books, etc., etc. These two kinds of objects really have no significant direct interaction. But to check statements made about objects (b), we have to refer to statements made about objects (a). In some peculiar way a book review is inseparable from the book that it reviews (without the latter, it would have no meaning). Moreover, the book review has some informational value about such a book. (Indeed, it may even "influence" the structure-function of subsequently written books.) So a book review is only linguistically separated from the book to which it refers. It is also only linguistically united with this book. To review a history book implies a different set of conditions than to review a novel. The form and style required for these reviews is different (but complementary in some peculiar way).

Because the quantum algorithm refers to a "metalanguage" it cannot have an unambiguous relationship to the "language" of classical physics. (e.g., a book review cannot refer unambiguously to the events described in the book that it is reviewing.) It can however refer to these events statistically (e.g., one could have a review of a whole category of books, saying that in 60% of novels of this kind, the hero triumphs over obstacles, etc.)

So let us say that the "novel" is told in the "classical" style of language, while the quantum style of "metalanguage" is able only to "review" the general statistical features of the "actual story of what happens" that has to be expressed the classical style.

I think you should somehow incorporate such notions into the introduction.

Page 9, middle paragraph. I would leave this out, as it is confusing to bring in classical physics, after we have clearly rejected it.

I would say instead that here, Bohr's linguistic thesis is plain.

Firstly, Rules Q_1 and Q_2 show that the incompatibility of different kinds of definite results in the algorithm (when two operators don't commute) corresponds to the incompatibility of experimental conditions needed to observe these results. Then Rule B merely asserts that the "metalanguage" of Hilbert space is only statistically related to the "language" of directly observable results.

In connection with Q_1 and Q_2, one must emphasize that the results of a measurement are classically describable. So Q_1 connects certain aspects of the algorithm with the unambiguously definable classical possibilities. Q_2 then shows that these possibilities cannot all be realized together (i.e., they are incompatible).

(A) refers to the relationship between the "metalanguage" and time (Dynamics).
(B) refers to the relationship between the "metalanguage" and the "language" (Linguistics).
But all this is based on the assumption of classical "language".

I think it is wrong to criticize this position (SEE POSTSCRIPT). Rather, it is necessary only to question whether or not it is inevitable. Emphasize here that another position might lead to new kinds of experimental and theoretical developments, that could be overlooked if we adopted Bohr's position without realizing that it is only one of several possible points of view (Refer here to Sect. 5).

I would also bring the point in at the end of Sect. 1.

In the hidden variable approach, then, we do not adopt this linguistic approach. Rather, we say:

(a) There are hidden variables determining actual results in each case – averaging over them is what yields the statistics.

(b) But we do not adopt the classical view that what can be observed in a measurement process is in principle separable from the total set of experimental conditions (or of the environmental conditions in general, whether part of an experiment or not). Rather we keep the feature of "wholeness" – but ontologically and not through epistemological assumptions.

So we not only introduce hidden variables, λ, but also assume that there will be a single rule for all changes in (ψ, λ) (rather than two rules, one "dynamic" and the other "linguistic"). But this rule is neither dynamical nor linguistical. Rather, it is a new kind of rule, having some of the features of both, but equivalent to neither.

It is similar to dynamics, in that there will be an equation of motion, determining changes of (ψ, λ) in each case, so that qu. mechanical results follow statistically by averaging over hidden parameters, λ.

It is trans-dynamical, in the sense that this equation of motion will depend on the whole environment, including the experimental apparatus as a special case. We assume that the "isolated" electron is treated approx. by Schrödinger's equation. But in any environment, (including that needed to make a measurement), there are additional terms, leading to a rule that will relate a definite final state to each initial state, but in a way that depends on the large-scale environment.

Refer here to Mach's principle, which relates inertial properties (differential equ. of motion of a free particle) of a micro system to cosmological environment. Our idea is in some ways similar, in that we relate the equation of motion of a quantum system to the whole environment, in such a way that an isolated particle is described by Schrödinger's equation. However, instead of bringing in only the cosmological environment, we bring in the whole environment on many levels, including that on the level of laboratory apparatus.

Now, I would give the example on P.10. I would revise the material on Pages 11 and 12, in the light of what has been said. On page 12, I would emphasize that because the micro laws depend on all the levels, the laws of the micro-level transcend the framework of dynamics, as we have known it. Because the "rule" (given on top of P.10) follows from a dependence of micro-laws on the environment determined at the level of the laboratory, it follows that we need an ordered hierarchy of inter-

related concepts to describe all the levels, which are all just aspects of one total indivisible structure, which functions as a single whole. The rule on top of P.10. then relates the functioning at the microlevel to certain features of the structure at the macro-level. So in a certain way, it has a structural relationship similar to that of language-metalanguage, used in Bohr's point of view. But it is now an ontological structure, and not a structure of language.

So it retains the feature of wholeness, but does not share with Bohr the notion that only classical concepts are unambiguously definable. Rather, it uses new unambiguous concepts (ψ, λ, the state of the large-scale environment, etc.). As shown in Sec 5, these new concepts imply the possibility of new unambiguously observable results.

I would rewrite all the material up to P.13 as one new unit, starting out afresh, if I were you.

Page 22, bottom. I would emphasize here that this refutes Bohr's thesis that the only unambiguously communicable features of what we observe correspond to classical concepts or some refinement of them.

I would here the importance of considering alternative axiomatic and conceptual structures even before the old ones are definitely refuted. For these new structures suggest new experiments that we would be unlikely to consider, otherwise. They suggest new ways to falsify current theories. A theory is always improved by the existence of a counter-theory, showing what it would mean to falsify it. If it is then falsified by experiment, we have evidently learned something new. And if not, we have also learned more about the domain of validity of the older theory. So we win, no matter what happens. Failure to consider alternatives tends to lead to a tacit form of dogmatism, because the older theory now seems inevitable, in the absence of an awareness of alternative possibilities.

As far as the second article is concerned, I think it is OK as it stands.

With best regards

David Bohm

P.S. With regard to Page 9, I would say that if you want to criticize Bohr, it is best to question his assumption that the only possible unambiguous concepts are those of classical physics, or of refinements of classical physics. One could say that this seems like an arbitrary assumption. In addition, emphasize that such an assumption, if unfounded, might be very restrictive, preventing us from developing new kinds of unambiguous concepts that are potentially of experimental significance (Referring here to Sec 5).

PPS

With regard to the whole paper, it would be useful to keep in mind the following general picture.

In everyday experience and in common perception, we have learned to analyze the world as constituted of objects, each located somewhere in space, and moving in a continuous way in time. This is what Bohr calls the space-time description.

Then we have learned to suppose that objects act on each other causally, to influence and determine each other's motions. This is the causal description.

Everyday experience is based on the tacit assumption that these two descriptions are compatible. Thus, we say that causes determine later effects, through forces and their effects on movement. We see no reason why a determination of causes is ruled out by a determination of where objects are and how they move.

The ability to apply both descriptions together leads, when refined in a quantitative way to the general scheme of classical dynamics. This scheme has two roles.

(a) Description of motion, position and of forces (Space-time and causal).

(b) The use of descriptions of force to make inferences about motion and the use of descriptions of position and motion to make inferences about forces.

Evidently, the inferential possibilities of classical dynamics depend on the simultaneous applicability of space-time and causal descriptions.

Finally, there is the division of observer and observed, subject and object. In everyday life, one of the objects in the whole field is singled out and called "I", "me", "the self", "my body", "the observer", etc. Because of the assumption of classical dynamics, we suppose its interactions with other objects can be taken into account.

In particular we may also set up a special object called the "observing apparatus", which serves as an extension of the "observer". The apparatus is so related to the human body that neither the behavior of the "observed object" nor the registry of this behaviour in the apparatus is significantly influenced by what the human observer does.

Now, the implication of the "quantum" is that this whole scheme breaks down. One finds that the experimental conditions needed for a precise causal description are incompatible with those needed for a precise space-time description. Therefore inferences of unlimited precision are no longer possible (The limits of precision being given by the uncertainty principle). Heisenberg's picture of an electron as one object and a microscope as another becomes inapplicable. The appearance of statistical fluctuations in behavior of the "observed result" merely expresses the breakdown of the possibility of drawing precise inferences, due to incompatibility of space-time and causal descriptions, needed for such inferences.

The division of apparatus and electron is now gone. Instead, we have two levels of language. (a) The descriptive languages of classical physics (space-time and causal), not applicable together in a completely unambiguous way. (b) Inferences can now be made only statistically. The inferences are drawn from the quantum metalanguage, as I explained earlier. The metalanguage now describes the "quantum object", which involves electron and observing apparatus in an inseparable way.

In a way, we can regard the classical phenomena as the "subject" (These are observed directly by the human "subject"). Then the "quantum object" is abstracted from these phenomena. Just as everyday "objects" are abstracted as invariant relationships holding in the "event level" where there are many "phenomena", so the "quantum object" is abstracted from a collection of "events" (e.g., readings of Geiger counters). So there is no analytic relationship between quantum object and individual phenomena. The quantum object belongs only to the ensemble.

Suppose you look at a picture, made of dots. With a few dots you get a vague picture. With more dots, the picture grows sharper. The picture is abstracted from the dots. There is no analytic relationship between the picture and the dots. Rather, it is statistical.

You could look on the interference pattern and quantum mechanics in a similar way.

Our criticism of Bohr is that he insists on the classical language as the only unambiguous one. We suggest another language, retaining "wholeness", but with a different ontology. It has new experimental consequences worth exploring.

In addition, it has new conceptual features that seem interesting and worthy of further study.

(a) A multi-level theory, in which all reflects all.

(b) Observation is inseparable from the reflective process. So that we don't say that what is to be observed is "there", apart from the process in which it can be observed.

(c) Yet, we don't say with Bohr that the process is incapable of unambiguous description.

(d) However, to describe it, we need new non-classical concepts (e.g., multi-level theory, structure-function, reflective function, etc.).

Jan 13, 1968

Dear Jeff,

I just received your long letter.

First, as to the job situation, it is much worse here than in the USA. There is an absolute freeze. Even some new appointments that had already been scheduled for physics dep't at Birkbeck have been cut off, though the men had been informally promised jobs. University budgets are being drastically slashed. I am really sorry, but I don't see what can be done here. I will write to Eugene Gross[2] on your behalf in the next few days. I do hope that something can be found for you. I realize how difficult your problem is. Couldn't you get a year's extension where you now are?

About the contents of your letter, it is difficult to comment on them. Probably one can learn from our correspondence that when there is a failure of communication, nothing is more useless than to try to discuss this failure. This is the surest way ever discovered to "confound the confusion". Perhaps we had better leave it at the simple fact that you and I have different relevance judgements, too different to be discussed by the inadequate content afforded by letters. Maybe in direct verbal discussions, we could do better.

My own experience in formalism is that to work in it requires so much attention that little energy is left to consider new informal notions. I have never been able to

[2] A physicist who was Bohm's research student at Princeton and remained on good terms. See Talbot (2017) for more detail—CT.

develop new informal ideas while working in formalism. Probably most physicists are similar in this regard. When I look at "densely packed" formalism, such as that of J. and P., or K. and S., I feel that to understand it, I must in effect commit myself to its language. So I get caught in activity similar to trying to find out whether I used first or second grade butter on the watch[3] (i.e., K. and S. are first grade butter, but J. and P.[4] are second grade butter). However, I see that your relevance judgements in this regard are quite different.

It may well be that publication of articles, such as our discussions of the language of physics will in fact only confound the already very great confusion. Very possibly, there is no way to "clear it up" at all. Every effort to discuss it tends to make it worse. For example, your article may perhaps establish more harmony with the von Neumann school, but it tends by this very move to go out of harmony with what I am trying to say. I am not blaming anyone here. All of us are caught in this mess, and as we move, we sink deeper in it.

I'll try to comment more on your article later.

Best regards Dave

Jan 15, 1968[5]

[Postmark 14th added – CT.]

Dear Jeff

I am continuing the letter of a few days ago.

I have written to Eugene Gross and also to Dr. Low[6] in Israel. I feel that it would be very good if you could get a job in Israel, and get out of the generally unpleasant situation now prevailing in American science. I am very sorry that you have so much to worry about now in the way of getting a job. However, I can say that Dr. Low's letter to me indicated that they are favorably disposed to giving you a fellowship. Once you are there, you could probably locate some kind of more permanent position, perhaps in Tel Aviv or Haifa.

With regard to your letter, I think it is best to proceed calmly and slowly. We do have different notions of what is relevant, and these have first to be sifted out carefully, if we are not merely to add to the existing confusion. Then perhaps, both of us will be able to come to perceive new orders of relevance, in which we are in closer harmony.

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[3]Reference to Alice in Wonderland. The March Hare had put butter on the Mad Hatter's broken watch, and defended his action by claiming it was the best grade butter. See also C135, p. 230, n 2—CT.

[4]J. and P. refers to J.M. Jauch and C. Piron (see C132, p. 99, n 1). K. and S. refers to Simon B. Kochen and Ernst Specker who proved a mathematical theorem relating to hidden variables in 1967—CT.

[5]First letter with this date—CT.

[6]William Low (Ze'ev Lev) a noted Israeli physicist—CT.

Meanwhile, it may be useful to clarify my own notions of relevance a bit more. Please try simply to look at these, without acceptance or rejection, just staying with the fact that this is the content that happens to be relevant to me at the moment (as one simply registers the fact that the clouds in the sky happen at one moment to resemble a horse, and at another moment to resemble a mountain). Actually, no specified or particular content is important enough to get into a quarrel about it. For in a moment's perception, any content may be seen to have limited relevance, and in this very perception, a creative new ordering of relevance takes place.

Perhaps I could discuss what I think concerning the work of Cauchy. In my view, the relevance of this work is much less than that attributed to it generally by mathematicians. Time and time again, mathematicians have believed that they had produced ultimate and solid foundations for mathematics, only to discover later that these "foundations" were in turn just as unclear as the original mathematics "founded" on them. Because so much energy was expended on consolidating the foundations, little energy was left for creative new discoveries. For example, mathematicians could perhaps have discovered new algorithms, more suitable for scientific investigations than the calculus. These new algorithms would have made Cauchy's investigations rather uninteresting. Indeed, some of Leibniz's ideas on the calculus could already have led to new algorithms, making the whole question of continuity of functions not very relevant. This is, in fact, a direction that I am now pursuing. However, it could easily have been done a hundred years ago, if people had not regarded the calculus as something that had to be consolidated.

In my own work, I cannot see that what Cauchy (or others did) is very helpful. On the contrary, I recall how when I first learned mathematics, I was ready to try new methods and algorithms, but the teacher said I must be "rigorous". This demand for rigor had a very negative effect on my ability to think in original ways. I am sure it has a similar effect on most young people.

To me, it appears that mathematicians (like von Neumann) are putting quantum theory on what seems to be a solid formal basis, not noticing that the formal basis in turn rest on a very confused informal basis. It is like a man who builds his house on a solid concrete foundation, which is so extensive and deep that he loses sight of the fact that nothing supports the concrete except a quagmire. And indeed, nowhere in the world is there a "solid" foundation for physics. In such a situation, what is called for is something more like an airplane, a helicopter, or a dirigible than like a skyscraper erected on a solid granite box. In other words, it is only in free movement that one can have some measure of security. To armor oneself with "solid" foundations is as irrelevant as to adopt the armored structure of a dinosaur under modern conditions.

I get the feeling that you don't appreciate fully how serious is the confusion in v. Neumann's informal approach. As indicated in the articles with Schumacher, he assumes two existentially disjoint domains, classical and quantum, in interaction. As we point out in detail in the articles, there is no way to do this that is free of confusion. And there is no way to be sure that this confusion in informal language does not confuse what is done with the formalism. (Indeed, we indicate that it does confuse what is done with the formalism.) That is why I say that such work is like erecting solid concrete "foundations" in a quagmire. In certain ways, the formal "clarification"

makes the situation more dangerous than ever, because it takes attention away from the fact that all that is said and done has, after all, no foundation.

If you are really serious about putting quantum theory on a "solid" foundation, I suggest that what it entails is for you to develop a consistent (confusion free) informal language, for discussing formal theories of the von Neumann kind. Otherwise, I feel that differences such as those between K. and S., J. and P.[7] , etc. will be as irrelevant as differences between the grade of butter in the running of watches. For ultimately, all these "solid" foundations are entangled in the confusion in the informal language. So no matter how clear K. and S. may be in their mathematics, one gets lost in trying to see what it means in the context of physics. Or even worse, one may lose sight of the fact that there is an informal context that is relevant, even to what is done with the formalism.

You say that changes in the informal language are arbitrary, thereby implying that, in some way, the formal language is not arbitrary. However, in my view, the formalism is equally arbitrary (a better word would be "fortuitous").

Mathematicians and physicists tacitly tried formalisms as solid and well established structures. But actually, these are fortuitously dependent on the personalities of those who are generally recognised to be authorities and leaders in the field (which personalities in turn depend fortuitously on the social situation, the whole historical evolution, etc.). Thus, if v. Neumann, Hilbert, Cauchy and a few others had had different personalities, the whole formalism of mathematics (as used in physics) could have been radically different from what it is today. What I find so hard to communicate is the insubstantial, "vaporous" character of formal structures. They can change as easily as can the shapes seen in clouds, and have no more ultimate significance than these shapes. One man sees a horse, and another a mountain. Likewise, one man sees the differential calculus and another sees purely algebraic structures, in which continuity is irrelevant. Why do scientists take these formal structures so seriously? It is like trying to put solid foundations under the "castles in the sky", that can be seen in the clouds. Perhaps the explanation is that most people learn very early to believe that they cannot do really creative work, that they lack a special quality called "genius", that would be needed for such work. So they restrict themselves to trying to clarify (or put solid foundations under) the shapes seen in the clouds, by other men, who for fortuitous reasons, are somewhat less conditioned to believe in the limitations that they are supposed to have. Perhaps some day, people will see that "anybody can do it". It doesn't take any special "genius", whose work is then regarded as an authoritative and solid structure of ideas (or capable of being made so, by people of more limited abilities).

I hope you will remember that all that I write (and all that I have ever written) is just a set of relevance judgements, no more substantial than the shapes in the clouds. (Remember also that this holds for v. Neumann, K. and S., J. and P., Newton, Einstein, Bohr, etc.)

[7] See p. 205, n 4—CT.

I'll continue more later

Best regards

Dave

Jan 15 , 1968[8]

[Postmark 17th added – CT.]

Dear Jeff

This is to continue the letter of a few days ago.

As I indicated, you and I have different relevance judgements in a number of issues. Very probably, this difference has been accentuated since we have been going our separate ways, each working on his own lines. Nevertheless, it was also almost certainly always there.

Roughly, you feel that informal discussion is arbitrary, subject to fortuitous variations, dependent on subjective features (perhaps of the Ego), while the formalism is the opposite, i.e. necessary, and not dependent on subjective features (of the Ego). So it could perhaps be said that you have some sort of notion of the relevance of mathematical truth, or of truth being in some sense mathematical. On the other hand, as explained in the previous letter, I feel that the formalism has been determined largely by fortuitous features of the personalities of those who happen to be recognised as leaders in the field. So in my view, it is mainly the Ego which has been operating to determine what is to be regarded as relevant, by elaborating certain kinds of formalism. However, because of the heavy emphasis on logical deduction and proof, it has been made to appear that all is necessary and true, so that the Ego has nothing to do with it. What is overlooked here is that formalisms can change as easily as the shapes of clouds. It is the Ego which holds to one "shape" as relevant instead of another. Once a certain formal structure is thus fixed, one can be very logical and clear. Nevertheless, it may all be irrelevant, because genuine clarity often requires that the whole formalism be set aside in favor of a novel one. Thus, the emphasis on proofs and clear logical deductions can easily cover up a deep confusion around the question of why we work with one formalism all the time, instead of frequently exploring novel formalisms. So in my view, mathematics is not a kind of truth. It is a formal form of description. It can be neither true nor false. Rather, it requires perception in each situation to see its relevance and irrelevance. This is possible only if formalisms are freely alterable all the time.

It may be that Einstein had notions of mathematical truth somewhat similar to those described above. But in my view, this was a serious barrier to his progress in the later years of his life. In a few years around 1905, 1906, etc., he wrote papers on three novel and original subjects, Special Relativity, Quantum Theory and Brownian motion. If you read these [you] will be struck by the simplicity of his reasoning,

[8] Second letter with this date—CT.

by its basically informal character, by its freedom from complicated and confusing chains of formal reasoning, that are full of unsolved problems and ambiguous terms of discussion. Later, being influenced by Minkowski and Levi-Civita, he went over to formal geometrical forms, with tensor analysis, matrices, curvature, etc., etc. Although he developed an interesting theory, general relativity, in this way, he was never satisfied with it. He felt the need to go on to seek a unified field theory. But in 30 or so years of work, he went only from one formalism to another, never arriving at new discoveries. Each formalism involved non linear equations that could not be solved, and whose terms were therefore ambiguous in meaning, because one never knew in what way they could be relevant to what Bohr called the experimental conditions and the experimental results. How different this was from the simple, direct, and basically informal character of the work of the fruitful early years of his life. Perhaps if he had never been blinded by the dazzle of tensor analysis, he could have gone on making simple but deeply significant informal discoveries, instead of spending 30 years in a fruitless search for unified field theories, of a formal kind.

Perhaps you will see from this that I feel that only when you start from the informal and then feel your way slowly to the formal do you keep out of the trap of allowing the Ego to determine relevance judgements. When you start from the informal, you are sensitive to the actual situation under investigation. It is like the artist who first makes a general sketch and then articulate the details. But if you start with the formal, your choice of formalism can only be determined by the conditioning, which is the Ego.

Consider, for example, those followers of v. Neumann who want to base everything on a formal algebra or lattice of some kind, involving the possible answers to "Yes-No" questions. Why do they regard this approach as relevant? If you do an experiment, how do you observe a "Yes-No" question? Evidently, this cannot be done. The question is imposed by the theoretician. To discuss an experiment, you have to describe the experimental conditions, as well as the experimental results. Until it is possible to do this in terms of "Yes-No" questions, there is no way to connect up the formalism to experiment and observation, except by the arbitrary fiat the Ego of the theoretician, who verbally says that an experiment can be treated as a set of Yes-No questions. He says this because it makes him feel good to believe that he has solved the problem of measurement.

If the relationship of theory to the informal experimental situation is confused, this can confuse all that is done with the formalism. While one is occupied with proofs, deductions, etc., one is able to fail to notice that there is no foundation to efforts to regard what is done as more than pure mathematics. Meanwhile, attention is thus diverted from cues to changes of informal and formal languages, that are given by a consideration of the total situation, in which there is no feeling that one has to give special relevance to a particular formalism.

Perhaps I could illustrate my views here by saying that the quantum formalism is now dead, so that it is not interesting whether it is logically impeccable or not. In the early days, it was alive and full of new discoveries, though formally, it was not very "clear" what was being done. After it was "cleared up" by Dirac, v. Neumann, etc., new discoveries almost ceased. Now 40 years later, even the donors of research

funds are beginning to question whether "elementary particle physics" is worth all the money that it costs. The note of progress seems to have gone down, in a kind of inverse proportion to the number of physicists working in the field, and to their technical competence in mathematical formalism. Is this nothing but an accident? In my view it is not an accident at all.

Now, it is clear that our relevance judgements are very different. What are we to do? Is it of any use for you or me to try to convince each other with proofs, arguments, refutations, etc.? Evidently not. For whatever I say has no logical weight with you at all, if my premises are not relevant to you (and of course, vice versa). As long as each of us holds to fixed relevance judgements, we cannot meet. If we do this, it is best that we simply have nothing to do with each other. For every attempt to argue or convince can do nothing but confuse the issue yet further.

This is all I meant by the statement to which you objected. "What is relevant is the irrelevance of holding to fixed judgements of relevance, especially when one wishes to communicate with others."

Of course, the phrasing of the above statement can be improved. I would rather say: "It is relevant in communication to be aware of all the judgements of relevance, one's own and those of the other, and of how fixing these tends to get in the way of communication."

However I did not mean this in the form of an edict, imposed by a mythical entity called Bohmacher. If you object to this statement, let me know what your objections are. If you feel that fixed judgements of relevance are no barrier to communication, then nobody can possibly stop you from going on with them. However, I feel that specifically in our communication at present, fixed judgements of relevance <u>are</u> the main barrier. I am not suggesting a rule, whereby people are compelled to change their relevance judgements all the time. Rather, I suggest only that if you see the meaning of what I say, that is all that is needed to change what you do, in our correspondence (and elsewhere, of course).

Perhaps you are taking the verbal form of my statements too literally. Very probably, this form could be improved a great deal. But I also hoped that you could "read between the lines", so that you could go beyond inadequacies in my form of communication.

More later,

Best regards

Dave

P.S. I notice you use the word "guilty" a lot in your letter. In my view, this is another irrelevant notion.

In some way, both of us are responsible for the failure of communication between us, in the sense that <u>what we did</u> brought it about and that <u>what we do from now on</u> determines whether it will continue or not. But to <u>blame</u> anyone, or say that he is <u>guilty</u> has no meaning in this context.

After all, nobody <u>chooses</u> his relevance judgements. They "just happen", as the rain "just falls", rather than, for example, because God <u>chose</u> to make it fall. Every

person makes such judgements according to his total conditioning. A new understanding, a new awareness, makes him effectively a different person, who cannot do other than make correspondingly different relevance judgements.

Indeed, even in simpler contexts (e.g., getting up from a chair), one does not really choose or will to do it. If, for example, you ask yourself "Just when and how did I choose to get up?", you will see that generally speaking there was never actually a moment of choice. It "just happened", step by step. Later, we verbally say: "I made a choice to do so." This projects a little "drama" in thought of "The man who chose or willed to get up." But this drama is irrelevant, except for giving one the pleasure of believing that one is an entity who has power to exert will or choice.

So it makes no sense to say that someone is guilty or to be blamed for what he did. Whatever he did, he had the feeling of its relevance, and therefore, he could not have done otherwise. Blaming a person will never change him significantly. It is only when one deeply understands and perceives the relevance and irrelevance of his judgements of relevance that his actions can change fundamentally. And then, it will happen of its own accord, without choice or efforts of will.

Similarly, it makes no sense to praise someone for what he did. Whatever he did, it flowed from what was relevant to him at the time, and he could not have done otherwise.

Perhaps you misunderstood my letter, and thought I was blaming you for the way you wrote your article. If so, please let me correct this wrong impression right now. I meant to say that first of all, there was a lot of interesting and relevant content in your article, but that, because it brought together so many mutually irrelevant points of view, the net effect was to add to the confusion that already exists on the subject. This confusion was your responsibility, but of course, not entirely yours. For the whole subject is now so confused that any attempt to refer to it tends to fall into similar confusion. Thus, you must harmonize with whatever you refer to. But if you try to "harmonize with confusion", then you too will be confused.

When I asked you why you wanted to write an article while you are in a state of disharmony, this was not a criticism or any kind of blame, but just a simple question, that I thought you would find it interesting to consider and perhaps even to "meditate" upon. I did not wish to imply that I have done better than you in this regard. The question is relevant to me too and indeed to everybody.

I am not blaming J. and P., von Neumann, or anyone else. Each was responsible for what he did, but could not have done otherwise, without a deeper understanding of why he did what he did.

It is useful to keep in mind that deeply, no one acts from predetermined intentions (though in some superficial cases, like getting up from a chair, this notion can sometimes have a limited kind of relevance, in the sense that I can say I want to get up, and then this is followed by the action of getting up). None of us intended to adopt the views in physics or mathematics that he now has. It "just happened", because of an infinite complex of factors, mostly unknown. So even if you feel my relevance judgements to be irrelevant, please recall that it was never my intention to adopt them. Rather, they are operative, because to me, they are relevant.

In short, then, relevance is no kind of value. To give value to relevance judgements leads to confusion. If we give positive value to them, we will want to keep them, even in contexts where they are not relevant. If we give negative value to them, will want to get rid of them, even in contexts where they are relevant. So relevance judgements cannot relevantly be valued. Rather, it is relevant to be aware of the irrelevance of giving value to them.

Jan 25th 1968

[Date added – CT.]

Dear Jeff

I wonder if you received my last two letters (Dec 24 and a second sometime in January, where I gave detailed comments on your articles).

I hope your wife and son are doing well.

Enclosed is a copy of an article which I received.

Best regards

David Bohm

PS. Did you get a manuscript of mine entitled "Some Further Remarks on Order for the Bellagio Conference in Theoretical Biology"?[9]

Feb 12th 1968
Date stamp on envelope 14th
[Date and comment added – CT.]

Dear Jeff

Thanks a lot for your letter and article.[10] On the whole it seems good. I enclose some comments.

Best regards

David Bohm

Comments on Article

Page 5. Middle.

I think that the confusion in Heisenberg's analysis is worth a whole paragraph. Heisenberg treats the observing apparatus and electron as two different and separately existent objects in interaction, exactly as is done in the classical ontology. But then he uses the quantum theory to treat this interaction process. The quantum theory,

[9]Waddington (1969), pp. 41–60. See Introduction, p. 3, n 10 for more details. Also C132, p. 147, n 14—CT.

[10]Published as Bub (1969)—CT

as used by Heisenberg, is in contradiction with the diagram of the microscope and the electron, that is so familiar to everyone. Therefore, the argument is confused. At most, it is useful to illustrate the limits of applicability of classical concepts. But beyond this, it plays no real role whatsoever in the theory. Unfortunately, most texts either tacitly or explicitly assume that Heisenberg's argument is both relevant and basic, whereas its relevance is actually extremely limited, and, being in contradiction with the theory, it can hardly at the same time, be a basic aspect of the theory.

The contradiction I mentioned above is clearly asserted by Bohr. He said that the "quantum object" is on another level from the classical objects and events that we can actually observe and communicate about in an "unambiguous" way. Bohr never actually used Heisenberg's diagram, and this is probably not an accident. According to Bohr, the "quantum object" is abstracted statistically from a vast number of classically observable phenomena of a similar nature (as the picture in a book emerges as an abstraction from the printed dots). So the quantum object is inseparable from the whole set of experimental conditions.

I would say that Bohr's notion of the relationship of observed phenomena and of lower level "objects" may well be a fruitful one. But, of course, his mistake is to assert that only classical concepts can be used in describing the observed phenomena unambiguously. If Bohr gave up this latter assumption, then it is not at all clear that we would be basically in conflict with him. Perhaps it would be worthwhile for you to assert this somewhere in the article.

On Schumacher's attitude

Schumacher agrees with us that Bohr is wrong to assume that classical concepts are the only unambiguous ones.

However, he emphasizes the extremely new and creative feature of Bohr's perception, that subject and object are different levels of communication about the same field. This gives a deep insight into this inseparable relationship.

Further discussion with Schumacher brought out that he wants to emphasize, not "language" or "linguistics", but communication in general (Visual, auditory, music, language, etc., etc.). We cannot separate the question of knowledge from that of communication. Knowledge is indeed an aspect of communication. I think he is as much against "linguistic philosophy" as we are. Nevertheless, some very deep questions concerning the meaning of language are raised, when we attempt verbally to assert that "reality is basically unverbalizable". He believes that to clarify this question is the key to "substantive progress".

About the question of randomness, he emphasizes the distinction between in its creative and analytic (logico-deductive) aspects. Current probability theories attempt to to ascribe a measure (of probability) to randomness. Schumacher thanks this is a contradiction, equivalent to the term "order of disorder". He thinks that randomness is basically incomplete self-definition of an order. Thus, it is, in a deep sense, another term meaning "potentially creative". The relationship of numerical probabilities to randomness is therefore, in his view, at present ascribed in a confused way. Clarifying this question would then be very important.

If we take this meaning of the term "randomness", we see that a book review is indeed "randomly" related to the books that are reviewed in it (i.e., it does not "substantively" define the order of the books that are reviewed). This does not necessarily imply a probability measure, relating the book review to statistical distributions of words in the books that are reviewed. Similarly, the "quantum object" is "randomly" related to the perceived physical events (measurements) that take place in the laboratory. Schumacher agrees that current probability measures applied to this "randomness" are confused, and not necessarily inevitable. So, tacitly at least, he could agree with us that more general measures could be applied to the contingences (i.e., results of individual measurements). Indeed, he is more radical, and believes that something new is needed, altogether beyond the notion of applying a measure to random (i.e., creative) sets of contingencies.

In short, what he wants to keep in Bohr is what he regards as deep, radical, revolutionary, and essential. This is the notion of not regarding physical concepts (e.g. Hilbert space) as being in a correspondence with "what is". Instead, the Hilbert space generates a field of "statements" about a vast set of contingencies, providing a way of asserting the operations of necessity in this field of contingencies. (Without contingency, necessity has no field to operate). But the necessity is not absolute. The notion of "randomness" allows for the creative emergence of new orders in the field of contingencies. (Classical physics would never have allowed for this.)

Schumacher regards Bohr's use of the correspondence principle (asserting that our unambiguous concepts are only classical) as an attempt to "mitigate" the extreme novelty of his basic views about the "quantum". He thinks they are inessential, and a source of confusion, in their present form. Thus, he is basically not in disagreement with what we want to say.

Page 6. Even in our point of view, quantum objects might well have a different status from large scale objects. Indeed, the structural process point of view implies that each "object" is an abstraction of a set of other objects of a different "status". These other objects are contingently related to the abstracted object in question.

Therefore, it seems important to assert our agreement with the deeper aspect of Bohr's thesis, while we assert our disagreement with the superficial aspect, in which he equates "unambiguous" with "classical". We should also say that unfortunately, Bohr's writings have suggested to most people that what is basic to his views is the necessity for all unambiguous concepts to be classical (or refinements). If we were to take Bohr's views on the mode of abstraction as basic and revolutionary and his views about classical concepts as superficial efforts to "mitigate" this novelty, then we would even say that hidden variables are deeply in agreement with what Bohr said.

How this comes about is as follows: The wave function (the quantum algorithm) is only contingently related to the observable events. The actual events depend on the total experimental set of conditions, which determine what "observable" is being measured. We differ from Bohr mainly in saying that the description both of the results of the experiments (and ultimately of the experimental conditions) involves concepts going beyond those of classical physics.

To be sure, the present version of the theory is reminiscent of classical mechanism, insofar as we assume a hidden variable, which obeys a deterministic equation. But we could quite consistently regard this as a provisional "bridge" to a deeper "structural-

process" approach. We could assert our readiness to abstract from our present theory a different approach, which drops the deterministic equation. Instead, we could (as Bohr does) define a field of contingent events or structures, observable on the large scale level. We could, however, differ from Bohr, in that this field would be unambiguously described by new non-classical concepts. We could then bring in new "micro" laws, which would assert a limited kind of necessity in this field of contingencies. This latter field would be "random" in Schumacher's sense. That is, the contingencies would not be completely self-defined in their order, so that there could be room in them for creative emergence of new orders, reflecting a "micro-level" operating in this field of contingencies.

To do this, we need to develop new non-classical concepts. The value of hidden variable theories is that they serve as a "bridge" to help indicate what these new concepts might be. This is in addition to their value in helping to show what it means to falsify the orthodox theories.

To be sure, it would be difficult to put this into your article. But if you could indicate something of this nature, the article would gain force and value.

Probably, it would be inappropriate to emphasize this question too much. But if you could, in different places, indicate that all sorts of theoretical ideas have to be considered, to relate to each other and to criticize each other, you could, without committing yourself to hidden variables, assert that consideration of them has a role to play in the overall dialectic of development of new physical theories. You could assert that Bohr's deeper views are not at all incompatible with the deep intention behind the development of hidden variable theories. Indeed, in the long run, the structural process point of view aims to realize a theory that is in essence compatible with Bohr's deeper intentions. What is necessary is to point out is how superficial is Bohr's emphasis on classical concepts. Don't fall into the trap of interpreting Bohr as being in opposition to you. You yourself said that "Bohr's philosophy does not really exist". So why don't you too "play the game" and re-interpret Bohr so as to agree with you. In my view, you are closer to the essence of Bohr than Heisenberg and most physicists are. If you were to say that the essence of Bohr is compatible with the deep intention behind hidden variables (and that he is only superficially against it) this might generate so much surprise that the reader's attention would be awakened, so as to question the whole story. This is really what is to be desired.

Page 30 (Conclusion).

If we take Bohr's "deeper" thesis, it implies neither "rockbound indeterminism" nor determinism. The very word "random" now means "creative". We all agree that every law must leave room for creative emergence of what is new. Bohr's notions make room for this in a radically new way. A given law does not determine a certain field of contingencies, but merely asserts some predispositions (or "propensities" as Popper would say) in the field of contingencies. (A "delta function" is the limiting case of complete necessity.) Therefore, to say that Hilbert space determines propensities in contingent sets of events leaves room for entirely different orders in these contingencies (e.g., those implied by hidden variables).

The "orthodox" view of Bohr's thesis is then that "randomness" implies current notions of probability measure – therefore "rockbound indeterminism". Notice that the algorithm of Hilbert space is only contingently related to the notion of probability measure. It could mean something else, but still a relationship in a large set (ensemble) of events. Our own view of Bohr's thesis is that randomness does not always mean probability measure, as defined in the orthodox view.

Rosenfeld and DLP have come out in favor of this "orthodox" view on probability measure. At the same time, they seem to wish to assert that these views on probability measure are alone enough to justify the scheme of quantum mechanics without radically new epistemological views (such as those of Bohr). This position is, of course, confused. But your last paragraph (P. 31) is also confused.

If you accept all of Bohr's theses, then as far as one can tell, the result is consistent. So although his theses are not inevitable, they are possible. And there is no logical reason compelling one to look for a new theses. As far as I can see, these reasons are tactical and methodological. We showed that Bohr's thesis is not inevitable. In view of the need to criticize older concepts with the aid of newer ones, why not explore our thesis (without commitment) as part of the dialectic of development of scientific theories? This exploration may well have the same deep intention as is behind Bohr's deep thesis (about "objects" of one level having a different status from "objects" of another level, in the sense that one is an abstraction applying "randomly" in the other, which latter is a field of contingencies relative to the former). On the other hand, the study of hidden variables does drop Bohr's superficial thesis about the classical concepts being the only unambiguous concepts.

The notion that classical concepts are an approximation to a quantum theory of measurements directly contradicts the whole of Bohr's thesis. But I wonder if it is of any use to say this here. Why not leave out altogether all mention of DLP in this article? After all, it will all come out in the next article. What you say here about DLP can only lead to confusion.

More remarks on Schumacher's ideas.

Schumacher regards the question of division of subject and object as basically a formal one, arising in the sphere of communications (including language as a special case). In other words, the notion of subject and object have no content. That is to say, there is no factual, objective significance to this division. Rather, it is a form imposed by our mode of communication.

This form is conventional. Historically, certain conventions have arisen. (e.g., the subject is "inside the skin" while the object is "outside the skin".) But entirely different conventions are possible, and indeed advantageous. Bohr would, for example, take the configuration of observable events as a kind of "subject", while the object is the "quantum system" abstracted from these. In the customary historical convention, subject and object are taken effectively as separate and distinct objects in interaction. In Bohr's view, subject and object interpenetrate inseparably, in a kind of "wholeness". The division is a formal one, for the purposes of communication.

You say that Bohr's views were physicalistic and not linguistic. But what is physics? Physics is defined by a set of conventions (largely tacit and historically conditioned).

Why are biology and psychology not taken to be part of physics? Evidently, their subject matter is interwoven inseparably. But people thought it convenient to abstract physics as a "distinct" subject.

It is important to expose these conventions to our view, because they could be getting in the way of clear and free thinking. Now, basically it is through communication (mainly by language) that physics is defined in a tacit manner. The division of subject and object is an essential aspect of this definition. The world is called the "object" and the "thinking subject" is conventionally said to be "observing" this world. Evidently, whatever is said to be examined by the subject is, by definition, some kind of object. By definition, the "real subject" can never examine himself, in his very act of carrying out the examination in question.

Thus the linguistic and conventional division of subject and object has defined what we mean by the words "scientific knowledge" and ipso facto, what we mean by the terms "physicalistic" and "linguistic". Bohr was, according to Schumacher, tacitly touching on these questions (which of course have nothing to do with linguistic philosophy).

It is important to note that Schumacher also regards the definition of words like "randomness" and "statistical" as historical conditional conventions, that could be altered, with advantage.

Questions of language can according to Schumacher, be important. The reason is that "what is the case" is asserted through linguistic forms, conventionally defined. When these forms are taken as universal, we are committed to analyses, whose structure is really conventional, but which seem to be inevitable. Nobody is primarily concerned with language. Yet, whether we like it or not, we will be primarily concerned with linguistic puzzles, unless we are clearly aware of the vast role of language, in what is called "fact" (e.g., the musician is not primarily concerned with notes, but if he ignores the effects of their structure, he will become the prisoner of this very structure).

You are quite right (and Schumacher agrees) to say that Bohr confused the issue by trying to salvage what was true of the classical conceptual structure and to build on it. But let us now say that this was a "superficial" part of Bohr's theses. The deeply revolutionary essence was in the need for a hierarchy of concepts, with the notion of a random (creative) relationship between different levels of the hierarchy (Probability measure is only a trivially restricted case of randomness). Other hierarchical views overlooked the creative (random) relationship of levels leading only to a more complex form of "ironbound determinism" or "ironbound indeterminism." The hierarchy of creatively related metaconcepts is not in itself restrictive. Rather, it opens up vast possibilities for development.

Bohr did lack a clear notion of creativity (The latter is too vast to be called a "concept"). But he had something like it implicit in some of his views.

The important point is not only that a conceptual framework can evolve creatively. Much deeper is the need to have a language structure that leaves room for such an evolution, without the need for a tremendous "crisis". The notion that "metaconcepts" apply "randomly" (creatively) helps provide such a structure.

General speaking, fluctuation phenomena are the field exhibiting the possibility of creation. If the metaconcepts are recognised explicitly to apply in the general statistical sense, this does not leave room for creation in each level. This is extremely important. For example, let us consider human perception. Here, the "subject" can usefully be defined by the convention that the subject is some set of mental "events" perceived at a very low level of abstraction. The "object" is than abstracted from these (as we can see very clearly on a dark night, when vaguely defined "objects" are always forming and dissolving). Evidently the "object" interpenetrates the "subject" in an inseparable way.

What is crucial here is to notice the "random" relation between object and subject. The "object" in no way provides a strict determination of the order of the "subjective" perceptual events. Rather, these are essentially free and creative. But in following their free creative mode of creation and annihilation, they subordinate themselves in certain very general and almost unspecifiable ways to the general pattern of the "object". In addition, they contain vast further orders, called "beauty", "harmony", "love", etc., etc. By convention, these have been said to belong to the "subject" and not to the "object". But it is entirely a matter of what is useful or convenient whether we say the beauty is in the object or in the subject. For after all, all subject and object are one, interpenetrating and inseparable.

Similarly, as Schumacher points out, in special relativity, the events observed in a certain frame of reference are the "subject", while the "object" is the set of "invariant relationships", abstracted from these events, and independent of Lorentz frame. Relativity theory defines the "object", but leaves the details of what happens in any frame to be completely free and potentially creative.

Likewise, in quantum theory, what happens at our perceptual level is free and creative. We can abstract all sorts of "objects" from this process, as perceived. The "quantum object" is one of these.

Don't you think this opens up a tremendous field for structural process? (It is really an extension of the notion of referential and inferential structures, to cover the whole of what we know and perceive.)

Feb 14, 1968

Dear Jeff

I just got a letter from Mary Hesse,[11] indicating that she is ready to publish your first article, but feels that the second is too mathematical for their journal. She suggests publishing the second article in another journal, but says it could perhaps be published along with the first, (though she evidently greatly prefers to do otherwise).

[11] Mary Hesse was a British philosopher of science, then editor of The British Journal for the Philosophy of Science (BJPS), to which Bub had submitted his articles—CT.

What do you think of publishing the second article in a more technical journal. You could add a paragraph or so of further introduction, to relate it to the first one.[12]

Best regards

David Bohm

PS. Enclosed is a notice about Krishnamurti that you may find interesting.

Feb 20, 1968

Dear Jeff

Thanks for your very interesting letter about Bohr and your various "experiences".

When I falsely recalled Bohr's statement as "trapped in language" instead of "suspended in language", this shows that, like you, I felt that Bohr's views imply a kind of imprisonment or fettering of the intelligence. However, as I indicated in my last letter to you, perhaps we can divide his views into his "deeper thesis" (subject and object as interpenetrating aspects of one set of creatively contingent phenomena) and his "superficial thesis" (only classical concepts are unambiguously communicable). If we reject his "superficial thesis", then perhaps your objections to Bohr would drop away. What do you say to this?

Of course, you are right to say that we can always change the language in terms of which phenomenon are to be described (so that we have a new kind of "plain language"). Nevertheless, the "language and metalanguage" question will arise once again, whenever we try to consider the conventions that distinguish subject and object. Of course, physics is by definition a field in which this distinction should not be relevant. Therefore, you are right to say that our language for observable phenomenon should be such that we do not need a metalanguage to talk about it. Rather, all these questions "beyond the language" are properly to be regarded as tacit rather than explicit. (Here, I am sure that Schumacher would agree too.) However, it is not at all certain that even in our "language" there could be no breakdown of Boolean logic. Isn't Boolean logic itself an idealization, based on our "plain language"? Your position amounts to the assumption that the foundation of physical language is in what is explicit and precisely definable. My view is that it is in the depths of the mind that are tacit and unspecifiable. The explicit emerges out of the tacit, as the precise image in the centre of vision emerges out of the vague background in the corner of the eye (as in the "psychedelic" image emerges out of a background of flux). In this regard, physics is not different from any other field of perception.

So in the last analysis, the specification of structural process may be tacit rather than explicit. It may well be a mistaken ideal to aim to associate each object (thing) with a Boolean lattice of its attributes, which are in principle measurable. Indeed, the whole notion of regarding measurement as basic to physics could be called into question. In a fundamental sense, measurement is very superficial, perhaps even a triviality.

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[12]The first article was in fact published in the BJPS, Bub (1968b), the second in the International Journal for Theoretical Physics, Bub (1969)—CT.

In other words, Bohr may be wrong in emphasizing what is unambiguously communicable in the foundation of physics. Rather, physics may be more like art, in that unambiguous communication emerges out of a vast background that is not precisely specifiable or measurable.

Now, about LSD and other such drugs, your letter was very interesting. It is hard to comment on your specific experiences, which I have never shared. However, I can say something on more general lines, that we do probably have in common.

Firstly, you question my view that such drugs "inhibit the inhibitions" due to thought, and thus "free" some other aspects of the functioning of the mind (such as the free flow of images). Here, let me call attention to the fact that studies of brain function have established the generality of this pattern, by which the various functions inhibit each other.

For example, we generally have only a relatively weak awareness of the sensations of our clothing, of sitting in the chair, etc. Some people, with certain brain damage in the thalamus, feel their clothing and such sensations with a maddening and intolerable intensity. It has been shown that these damaged sections of the thalamus normally inhibit critical awareness of such sensations. Indeed, by cutting out certain parts of the thalamus of animals, similar symptoms are produced.

All this is reminiscent of your descriptions of your own experiences. If you assume that the drug further inhibits your awareness of sensations of contact of the body with the ground or with the chair, this would explain the sensations of "floating in space" that you describe.

Although I haven't experimented with drugs, I do not find your description totally foreign to my experience. I see, in awareness, roughly three levels of perception.

1. Sense perception.
2. Emotional perception.
3. Intellectual perception.

Of the three, the last (intellectual) is clearly the slowest. I would say that emotional perception (beauty) is the fastest, while sense perception (form, color, etc.) is somewhere in between.

Perception is all one process. But its "spectrum" of frequencies is roughly analyzable in the above order. When awareness "emphasizes" one part of the spectrum, then attention is directed mainly to that part.

For example, when perception is mainly intellectual, awareness tends to focus on the "lower frequency" part of the "spectrum" of perception. Emotional perception (beauty) focuses mainly on the "highest frequency" parts of the "spectrum".

Therefore, when you are thinking, you are "soft-pedalling" all the faster processes. They are still there, but they are in the "background" of awareness. Likewise, there is a state of emotional perception, in which intellect is soft-pedalled, and there is a state of sensual awareness, in which both intellect and emotion are soft-pedalled.

I am led to assume that drugs like LSD and marijuana work by inhibiting the intellect, so that awareness focusses on the senses (and possibly on the emotions). You are right to distinguish them from alcohol, which probably inhibits almost everything. With these drugs, perception of process speeds up. This is quite inevitable, because the intellect, which works so slowly, has been inhibited.

There is another aspect of this process that is significant. We must distinguish, at sensual and emotional levels, between conditioned perception and unconditioned perception. Conditioned perception is structured by word and idea, as laid down in the past. Unconditioned perception is direct response of senses, emotion (and intellect) to the unknown, without the mediation of words or ideas, laid down in the past. Conditioned perception can still be very fast, even though the intellect is slow. We can compare the situation to a drama. The dramatist writes down a few words, quite slowly. From these words, the actors can elaborate a vast and complex set of actions, that take much less time to carry out than the dramatist took to think these words, and write them down. Yet, the actions are not spontaneous, free and independent. They are conditioned by the words of the dramatist. Similarly a few thoughts, very slow in themselves, can lead to the elaboration of a vast "dramatic" response at the sensual and emotional level. It is crucial to distinguish this "dramatic" response from the true unconditional perception that is a direct (unmediated) response to the unknown.

In reading four or five books written by people who have taken LSD, I have noted that one can trace most of the "visions" described by these people to verbal conditioning, which is is apparent from what they themselves say in their books. The "experiences" themselves are very fast, vital, colorful, and convincing. But I cannot escape the suspicion that they are mainly a "dramatization" of verbal conditioning rather than a direct contact with the unknown (e.g., of the kind that Krishnamurti talks about).

Some of what you describe strikes me as possibly being of a similar nature. For example, you became aware of the "Greekness" of the friend whom you mentioned. Isn't "Greekness" a purely verbal concept, based on past conditioning at an intellectual level? Wasn't your experience of "Greekness" perhaps a "dramatization" of this concept, at the sensual and emotional levels? Were you really in direct and immediate contact with this man as he really was, in his vast and unknown totality of eternal movement?

When the intellect is inhibited, one is able to see other aspects of the mind more clearly. So, in a way, something is gained. Yet, in the long run, perception conditioned by a drug is just as limited and meaningless as is perception conditioned by habits of thought. It doesn't reach out to totality, to the unknown depths that are immense and immeasurable.

One of the difficulties with such perception may well be that it is limited by the relative absence of intellect, so that it is unbalanced and incomplete. In particular, when thought does come in, one cannot avoid confusion.

For example, you describe how after a while, thoughts come in. "That was good", "I feel my clothes", "I must remember what it felt like", etc., etc. You tried to fight them off, but they got worse, and soon you come "down" from being "high".

Because your intellect was not very awake, it probably could not really understand that when you said "This effort to stop thought is just another stupid thought", this was a value judgement. Such a value judgement acts like a blast, like a bomb exploding in the middle of the the brain, to introduce utter confusion. The word "stupid" applied to anything is in itself a tremendous blow at the stem of the brain, which leaves one stupefied and confused. The effort of thought to stop thought is in itself a contradiction.

But you were treating it as a problem. Now a problem can be solved, so that to think of a problem is to force the brain to continue the thought of the problem, until a

solution emerges. But a contradiction cannot be solved. It has to be dropped. However, by treating a contradiction as a problem, the brain is caught in going on forever with the contradiction.

Had you been able to see this, you wouldn't have had to come "down". But in this case, perhaps you wouldn't have needed the drug in the first place. Basically, the "self" is a contradiction. But we are conditioned to treat it as a problem. So we try to "set it right". Since this is impossible, we are eternally trapped in thought at a very deep level. This very emphasis on thought in contradiction is a soft-pedalling of the faster emotional and sensual sides of awareness. (It also prevents real intellectual perception, which would be free of contradiction.) Therefore, what is called for is the ending of this contradiction, called the "self", which is mistaken for a "problem". The drug doesn't really do this. It apparently gives moments when the thought of the "self" is "soft-pedalled". But it is still there, in abeyance. It has not been dropped forever as the meaningless contradiction that it is. Unless this happens, playing with drugs seems to be just another "gimmick".

For this reason, I am inclined to doubt that these drugs can really teach us very much about the mind. Try to understand what is involved here. The mind, in each one of us, is deeply conditioned to contradiction, confusion, violence, and delusion. The drug alters the effects of this conditioning, without (as far as one can tell) altering the basic conditioning itself. How is it possible for a confused mind to know what it is really "learning" from the drug? Recall also that the "research worker" who "observes" someone else taking the drug is himself deeply, basically, and hopelessly confused. Whether he takes the drug himself or observes another who takes it, what is to be expected, starting from such confusion?

Doesn't it seem, rather, that what is called for is that each man shall begin to inquire into his own confusion? I suggest that when this confusion is gone, each man will be in immediate contact with something immensely beyond the "highest" experiences made available by any drug.

Best regards,

David Bohm

PS. I am looking forward to seeing you this summer.

Feb 26, 1968

Dear Jeff

Just received your letter of Feb 21. Briefly, I suggest that you modify your article[13] on the lines you propose. Change the title to include the notion of reconciliation. Put as much of the "new wine" in as you can, without making too much work for yourself.

[13] i.e. Bub (1968b)—CT.

The DLP issue is much less significant. Why not publish your reply to DLP in the same journal as DLP published their articles?[14] Begin it by summarizing your article on Bohr, and by emphasizing how DLP differ from Bohr, while they claim to be supporting him.

I'll reply to the rest of your letter later.

Best regards

David Bohm

PS. The new ideas could well be the basis of a further article, as you suggest.

March 8, 1968

Dear Jeff

Enclosed is a reply of mine to a contributor to the Bellagio Conference, which you may find interesting.

Best regards

D Bohm

March 4th, 1968

[Date added – CT.]

[There follows a type-written document: "Addendum to Remarks on Order for the Bellagio Conference on Theoretical Biology", which was published as "Addendum on Order and Neo-Darwinism" in Waddington (1969) pp 90-93. See Introduction, p 3, n 10 – CT.]

March 1, 1968

Dear Jeff

In your letter you asked what I thought about your notions of randomness as applied to the structural process underlying quantum theory.

On the whole, I would say that I agree with what you write on the subject. I would add a few points, however.

Perhaps we could regard the wave function as belonging to the quantum level of S-P (structural process). The dual function, ξ, may then be a kind of "image" of the quantum system in the scale large scale environment (including the apparatus). This is a bit like the idea that an electric charge produces an "image" in a metal sheet. If the sheet is a perfectly homogeneous conductor, the image is uniquely and necessarily related to

[14] This was published as Bub (1968a)—CT.

the charge. If it has inhomogeneities, these introduce a feature of contingency. As the "real" charge moves, its image jumps around in a somewhat fortuitous way (because the inhomogeneities are fortuitously related to the movement of the "real" charge).

Similarly, we may assume that ξ is an "image" of ψ that tends to move somewhat fortuitously in relationship to the latter. Current quantum mechanics assumes this relationship to be completely fortuitous or random. We can propose new theories, in which the "degree of fortuity" is limited, eventually approaching a relationship of simple necessity, as an extreme limit. Therefore, as you say, the role of ξ is to enable us to treat quantum theoretical randomness as potentially creative – i.e., capable of new orders emerging. (Read the Further Remarks on Order for the Bellagio Conference[15] for a discussion of how the fortuitous becoming the necessary is the basis of creativity).

If this is a correct view, then the temperature of the environment is what is crucial. At low temperatures, the fortuitous element in the environment is reduced. But of course, to get beyond qu. mchal randomness, we may need spatially homogeneous environment, as well as temporarily homogeneous process.

Best regards

David Bohm

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[15] Waddington (1969) pp. 41–60. See Introduction, p. 3, n 10—CT.